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ABSTRACT

This study undertakes to clarify the concept of a normative basis for curriculum research by considering four possible interpretations: (1) a set of substantive moral principles, (2) such a set taken as an integral part of a putative curriculum theory, (3) a meta-strategy governing curriculum decisionmaking, or (4) the norms which specify the kinds of terms that may be used in stating research hypotheses. None of these interpretations appears to be fully satisfactory for curriculum research, although the third and fourth (with qualifications) seem more promising than do the first and the second. (Author)

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WHAT IS A NORMATIVE BASIS FOR CURRICULUM RESEARCH?

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It is a fair assessment of the present state of meta-theoretical discussions in the social sciences to note that the movement to expose the inadequacies of positivist-inspired methodologies has taken firm hold. There has been particular dissatisfaction with the variants of behaviorism which still prevail in psychological and educational research. It is true that some of the reasons that have been given for rejecting behaviorism are based on theoretical misunderstanding. Such is the case, for instance, with the charge that behaviorism is an inhumane (to be distinguished from a nonhumanistic) methodology. But despite such shortcomings, many of the critics have rightly pointed out that (1) adherence to the methodological canons of behaviorism prevents the formulation of certain kinds of hypotheses which, if confirmed, would constitute important knowledge about human behavior and (2) behaviorist methodology is not value-free or value-neutral, contrary to what its adherents have supposed.

These two timely issues are frequently addressed in the NSSE 1972 yearbook, Philosophical Redirection of Educational Research. That the ideal of a value-free social science is logically impossible to realize is taken for granted by most of the contributors and therefore it is not surprising that rather than avoiding value questions several of the writers issue a call for the development or choice of a normative basis for educational research. However, the different ways in which the concept

of a normative basis for educational research can be interpreted has not been given systematic analysis despite the fact that the broader question which includes it--whether or not social science can be value-free--has been widely discussed. In the first part of this paper I attempt to develop several distinctions within the concept of a normative basis, taking for granted that at this level of generality what I say about educational research will apply to curriculum research as well. (Where special qualification is appropriate I shall provide it.) Specifically, in this part I shall first distinguish among various versions of the claim that social scientific inquiries can be value-free (hereafter referring to this claim by the abbreviation 'VF') concluding with an examination of the VF-thesis in its most defensible form. Next, I shall develop several different interpretations of 'normative basis'. Lastly, I shall undertake a detailed examination of the recent recommendations to formulate a normative basis for educational research, indicating in what respects I consider them to be mistaken or misleading. In Part II the conclusions established in Part I will be applied to an actual case of curriculum research--what has been called an empirical or naturalistic approach to curriculum.

I. Several preliminary points must be established before the VF-thesis can be profitably dealt with. First is the indeterminate nature of the concept of research. Many who are engaged in the collection of data wish to restrict the meaning of 'research' so as to exclude such activities as

the explication or rational reconstruction of the concepts which govern the inquiry. Those who disagree with this restriction are quick to point out that since some purely conceptual activity is indispensable for meaningful empirical investigation, it should be considered part of research. I shall opt for this wider conception of research and thus settle a dispute which is merely terminological.

Second, the word 'basis' in 'normative basis' suggests that normative considerations are not only distinct from, but are also logically or conceptually (perhaps even temporally) prior to descriptive or factual ones. But whether and to what extent such a dichotomy is justified is one of the contested issues in this paper and must therefore remain open at this point. I shall often use 'normative element' for 'normative basis' in order to avoid begging this question.

Third is the distinction between methodology and technique (or to use Gowin's expression, 'methods of work').[1] 'Methodology' refers to such elements of the corpus of scientific knowledge as the type of observation language employed, the manner of justification of statements or the criteria of acceptance for hypotheses. 'Technique' refers to such things as data gathering tools. To say that the social sciences differ in the techniques they employ is completely non-controversial; to say that they differ in methodology is to make a very controversial meta-theoretic claim.

I want now to register an important point which depends upon both what I have said about the meaning of 'research' and what I have said about the meaning of 'methodology'. Clearly, what the word 'methodology' means and what the methodology of a particular social science is at some stage in its development are two different things. I shall assume that an objective of educational research is to contribute to the development of educational theories. (A consideration of what type of theory, if any, is possible in education has to be deferred to a necessarily brief discussion later in this paper.) But, as most educators would agree, no educational theory containing well-confirmed empirical generalizations (whatever else it may include) has been produced to date. Therefore, to speak of the methodology of such a theory is at best to speculate about its logical and conceptual properties. There is, however, one important exception to this. The educational research on which the construction of a theory would have to depend must necessarily make some methodological presuppositions. That is, a methodological choice of type of observational categories or terms is inescapable. For instance, curriculum research to the extent that it serves to provide the foundations of curriculum theory has not yet advanced much beyond the development of such classificational schemata. We must conclude then that non-speculative talk about the methodology of educational (or curriculum) research at the present time is restricted to reference to the observational language features of social scientific methodology. ('Speculative' is not being used

pejoratively; in a longer, wider-ranging paper I would discuss the importance of those who, in advance of accumulation of a body of observations, have explored methodological structures appropriate only for developed theories. Their procedure has been to try to adapt to educational contexts various formal models developed in other disciplines.)[2]

Fourth, we must note that there is a difference in kind between methodological choices and choices of research goals extrinsic to a research methodology. The choice of type of observation terms to be employed in describing or picking out educational particulars is clearly methodological. Even though such methodological decisions determine how research goals are to be formulated (thus necessarily excluding alternative types of formulation), the other kind of choice still has to be made--to establish the hierarchy of research goals--since it is not possible to simultaneously pursue all goals whose formulation is compatible with a particular choice of observation terms or categories.

Fifth, methodological choices are inescapably normative, but whether or not they are inescapably moral as well is a controversial question to be answered in the discussion of the VF-thesis soon to follow. On the other hand, the choice of goals extrinsic to a research methodology is clearly both normative and moral--a point often conceded even by VF-advocates.

Sixth, the key distinction between the moral and the non-moral has been drawn in a variety of ways in the metaethical literature. I shall here follow Margolis and use 'moral' to characterize those values which

one takes to be overriding with respect to the conduct of one's life and the lives of others--i.e., values that take precedence over all alternative values.[3] I shall also take for granted that such overriding values cannot be discovered and that the differing sets of moral values that individuals or groups hold represent inescapably partisan choices which mark out ideals or visions of the worthwhile life that often conflict from one person to another. (It is important to keep in mind that this applies only to overriding values which are arrived at by appreciative judgment and not to norms which are conditional upon overriding values and can be justified as findings[4], given those particular values. Otherwise the domain of ultimate ethical disagreement will appear to be much larger than it actually is.) The domain of the non-moral is, of course, not restricted to non-moral values since it includes wholly non-normative statements which express neither appreciative judgments nor findings. It is important to realize that this way of distinguishing between value claims (appreciative judgments and findings) makes it possible for value judgments to be regarded as factual. (For a detailed and sophisticated explanation of how this undercuts the notorious is/ought problem see Margolis.[5]

With the help of the preceding stipulations and distinctions we are in a position to assess the merits of the VF-thesis in social science. My reason for tackling this heavily worked issue is that the development of a defensible position regarding the VF-thesis requires the pinpointing and classification of value elements that enter both the processes and products

of social science. Only such a comprehensive picture of the value spectrum will enable us to properly assess the recent calls for development of a normative basis for educational research.

A good way to move quickly to the heart of the VF-thesis and at the same time to avoid the gratuitous confusions that have surrounded this issue in the past is to list problems that most proponents of the VF-thesis concede to be not only normative but also moral. These are decisions about

(a) which research goals should be pursued,

(b) which research techniques should be adopted,

(c) how research projects should be staffed,

(d) how research findings should be disseminated, and credit for research accomplishments allocated.

Furthermore, tough-minded advocates of the VF-thesis have rightly held that the thesis is compatible with the facts that (e) values can be and often are the object of research and (f) the values that an investigator holds may and often do bias his results. In other words, to attack the VF-thesis by citing the presence of evaluative components in factors (a) - (f) is to attack a version that very few people would advance today.

The best way to state and defend the VF-thesis is to claim that the choice of the methodology of research (not to be confused with techniques or methods of research) need not, and in fact should not, depend upon moral norms but instead is subject only to epistemic criteria in the form of norms which encapsulate rules for the pursuit

of science as a corpus of theoretical knowledge. Such epistemic norms are very difficult to formulate explicitly since their content depends on a variably weighted combination of factors, the analysis of which is controversial. Usually cited in any list of such factors are predictive and explanatory fruitfulness, simplicity, and some measure of the relative weights of erroneous assertions that competing theories generate.[6] Difficulties of formulation aside, such epistemic norms are taken to be sufficient as standards of proof or criteria of acceptance of scientific hypotheses. In a recent article Jerome Popp has taken this position. He says:

> The attempt will be made to show that hypotheses may be accepted for more than one purpose, and that when the scientist qua scientist accepts a particular hypothesis he does so for a special purpose (or for certain special purposes). If such a distinction of purposes could be defended, then the scientific educational researcher could be seen as one who accepts and rejects hypotheses but yet is not required to make ethical judgments as part of this accepting or rejecting. In other words, his acceptance or rejection decisions could be seen as based upon an . . . . appeal to epistemic utilities.
>
> It seems clear that one can accept a particular hypothesis for practical purposes, i.e., action. This acceptance decision is made in light of the seriousness of error in the typically ethical sense. However, these practical purposes do not exhaust the possibilities. One could accept a hypothesis not because of its relevance to a particular practical decision but, rather because of its descriptive or explanatory value. That is to say, if the purpose of scientific inquiry is to construct explanatory laws and theories, then the educational researcher qua scientist must make judgments which, if they are to be considered acceptable, implement this purpose. Where these are value judgments they are epistemic value judgments.[7]

Popp's arguments have the effect of circumscribing the domain of scientific inquiry in such a way that certain arguments against the version of the VF-thesis that he maintains cannot consistently be raised. The version of the thesis that Popp defends assumes that what can properly be called scientific inquiry or someone's acting qua scientist is restricted to actions performed with the sole purpose of adding to the fund of systematic knowledge. We must note here that given the present state of development of theory in some social sciences, including education, inquiry with this motive rarely takes place in these areas. This observation, however, affects only the relevance and not the validity of Popp's arguments. Even if a researcher were to accept a hypothesis for reasons both of epistemic and of practical utility, his move, according to Popp, would have to be analyzed as a performance of two different actions--one an instance of scientific inquiry, the other not. Despite the oddity of this result, there is nothing conceptually incoherent about individuating actions in this way since the individuation of actions is a matter of convention anyway and simply requires a clear criterion. But the objection can be raised that when one accepts a hypothesis or when one chooses a set of observational categories for reasons of practical utility one is acting qua scientist as much as one is when one makes these decisions for reasons of epistemic utility alone. To agree with Popp when he says (citing Scriven) that "One may judge that an area of theoretical effort is not practically worthwhile, or that scientific conclusions are

being put to unworthy uses; but at the same time conclude that this is logically separate from the judgment that specific conclusions are unfounded and should be rejected,"[8] is not to agree that judgments of the second sort are scientific whereas those of the first are not. In fact, Scriven himself has inveighed against such a separation of the scientific and the moral, at least in the case of social science research where the roles of citizen-executive and scientist are fused.[9] Thus the distinction between epistemological (theoretical) and practical motives fails to support the role distinction between researcher qua scientist and researcher qua social policy-maker.

But this is not the whole story. A strong objection to the version of the VF-thesis that Popp defends (scientific inquiry is morally neutral without being free of non-moral norms) is the claim that the very choice of observational predicates that the social scientist uses to pick out or describe social particulars (e.g., institutions, their role bearers and the actions they perform) is necessarily a moral choice. For example, whether certain actions of students in an educational context are described by using a predicate such as 'hyperactive' on the one hand, or instead such predicates as 'labile' or 'reactive,' on the other, affects the very formulation of the research problem, and very likely has an impact on whether the conclusions of research which employs one or another set of categories favors the interests of one or another subgroup of the student population. It would seem that if a researcher either knows, or is in a position to find out, the consequences of alternative choices of descriptive categories, and if bringing about certain of these

consequences rather than others is not a matter of indifference from the standpoint of society's or the researcher's overriding values, then it must be said that the researcher is in fact faced with a moral choice, however he personally, may conceive it.

If this is so, then the distinction between moral and non-moral methodological choices also fails to correspond to or be supportable with the distinction between types of motive. Instead it will be seen to rest on the sort of overriding values the researcher holds and the amount of relevant information available to him. There is a difference between a moral choice and a choice which turns out to have moral consequences. In order for a choice to be an instance of the first it is not only necessary that alternative sets of consequences not be a matter of indifference from the standpoint of one's overriding values but also that one be in a position to come to know this before having to make the choice. By contrast, a great many choices that we make and which we are justified in believing are free of moral consequences turn out, as a matter of fact to have moral consequences which the actor was, at the time, in no position to predict or even to suspect. But even if this distinction can be maintained, it will not be possible neatly to distinguish in advance between non-moral and moral choices of observational categories in social scientific or in educational research because sometimes we decide only in retrospect what considerations should have governed our choices had we only been sensitive enough. All that we are able to conclude in advance is that it is unlikely that broad

methodological choices such as that between behaviorist and intentionalist observation languages are moral choices. Although it is virtually certain that such choices have consequences that are construable as moral from the standpoint of somebody's overriding values, we are rarely, if ever, in a position to find out exactly what these consequences are before the choice is made. Narrower choices, such as the selection of a particular finite set of descriptive categories to be employed in some educational research project, are, on this analysis, unlikely to be non-moral since they can usually be shown to depend upon some concept of education which in turn can be shown to be conditional upon, or even part of, some essentially partisan set of overriding values. A philosophical analysis of "our concept of education" which was alleged to be morally neutral and had gained wide acceptance in Anglo-American educational circles has recently been conceded by its author to have morally partisan features.[10] This is very likely the case with all other such analyses purporting to be value-free.

To point to the value-laden character of any concept of education is not in any way to diminish the importance of finding out its normative content. (This would presumably be done by charting the relations that are claimed to hold between education, knowledge, social class, indoctrination, character and vocation, as well as other concepts.) But it is quite another thing to claim or imply that the <u>methodology</u> of educational research ought to embody some set of substantial moral commitments regarding what is educational and what is not. This

recommendation seems to have received some currency both in Great Britain and the United States. A clear instance of it appears in a recent British article by G. Reddiford. He says:

> . . . not only is conceptual analysis of 'education' and related terms the indispensable preliminary to answering practical questions concerning curriculum . . . , it also sets limits to the lines educational research can and ought to take (my emphasis).[11]

Suggestions very similar to this were made by some of the contributors to the 1972 NSSE Yearbook, Philosophical Redirection of Educational Research. For example, in the Introduction written by the Yearbook Committee, there is proposed "a conception of educational research that would make this realm of investigation more normative, more significant, and more demanding,"[12] and later Gowin recommends that,

> The researcher should first try to be clear about the concepts, methods, and procedures of educational practice so as to be able to select phenomena to study that pass as educational phenomena and then adapt, invent, or utilize relevant research procedures.[13]

He points out that because what he recommends was not heeded in the past "many events which are educational never get studied now,"[14] and that his recommendation "is in contrast to the usual procedure which derives grounds for acceptable scientific research from the philosophy of science."[15] Gowin notes further that

> Many experts on research methodology claim that they are not even trying to decide about defensible educational phenomena. It seems to me, however, that selecting the phenomena of interest is one of the most

> crucial determinants of the eventual research product. I think this selection cannot be done without the conjoining of educational concepts with conceptions of methods of research (my emphasis).[16]

Dunkel, who is in agreement with Gowin on many points regarding the diagnosis of the ills of educational research, concludes his essay with the plea that

> Some kind of normative base must be found if education is to be more than a mindless technology, heeling in the breeze of every whim or rhetorical blast. The rather abortive state of curriculum and counseling--to name only two educational fields--is due in large part to their lack of an adequate normative base.[17]

Finally, Brian Holmes echoes Dunkel's thoughts when he points out that

> The important theories in educational research are normative in that they imply conceptions of culture and society. An integral part of empirical research is, in my view, prior conceptual analysis of these conceptions and theories. Without this we are charlatans--not scientists.[18]

Admittedly it is difficult to be certain that all of the writers quoted are making the same strong claim--that the methodology of educational research should embody some concept of education (which cannot but be a morally partisan concept). Holmes, for instance, may be making only the noncontroversial point that the choice of research goals, since it is a moral choice, should be examined to reveal what moral principles were presupposed in making it. This way of interpreting his remarks requires us to construe his phrase 'theories in educational research' as referring to some set of favored moral principles which an

educator believes may someday function in an educational theory when the empirical components of it are developed. However, if we interpret 'theories in educational research' to refer to the methodological presuppositions of research, then Holmes' claim that "they imply conceptions of culture and society" is false; not all methodological choices have moral significance as I have argued above.

Before commenting on the views of Reddiford, Gowin and Dunkel I want to list and explain the different sorts of normative elements that the indeterminate expression 'normative basis' could be used to refer to.

1. A normative basis for educational research may be the methodological presuppositions of research. I have discussed this alternative at great length already and shall say no more at this point.

2. A normative basis for educational research may be an educational theory which, if developed, would not only embody a methodology (thus including (1) above) but would also contain moral principles and empirical generalizations, thus making it possible to deduce predictions (which researchers would verify) and to justify decisions establishing research goal priorities. This is clearly an ideal which some educational researchers aspire to. Not only has no such theory been produced but it is a matter of considerable dispute whether such a theory is possible. Since a discussion of the nature of educational theory is well beyond the scope of this paper, I simply shall state my position on this issue without providing arguments for it. I believe that the arguments purporting

to show that an educational theory of the sort just sketched is not possible have failed to take into account certain recent alleged solutions of the is/ought problem in ethics. If we assume that this problem has been solved, then the objection to the possibility of a theory which contains both moral principles and non-normative statements has been met. There remains the difficulty that the use of intentional terms in observation statements of the theory makes it unlikely that we shall be able to establish empirical generalizations of any significant scope. As some have put it, a science of intention is impossible. This is true if the term 'science' is interpreted as hypothetico-deductive theorists do.[19] But it is not necessary to adhere to this interpretation. Instead, in social scientific areas one may be satisfied with a "weaker" notion of science, giving up, for instance, the deductivist principle of symmetry between explanation and prediction. Construing social science, including educational research, along these lines preserves the rationality of the enterprise and there seems to be no good reason to abstain from applying the term 'theory' (which is rather indeterminate in ordinary language anyway) to the body of principles and generalizations which guide research. In conclusion, I should note that some educators, realizing that there is still no educational theory of the sort described, may only be using the term 'normative basis' to refer to the set of moral principles that is a candidate for inclusion in some future educational theory. Earlier I suggested that this is one way to interpret Holmes's remarks.

3. An interpretation of 'normative basis for educational research' not very different from the preceding is to take it to refer to a loose "theory" consisting of either a vertical or horizontal array of practical syllogisms. I think that to characterize such an array of statements as a theory is unwarranted because it does not possess a full-fledged methodology, a necessary condition for calling something a theory. (I take it that the term 'methodology' is not as indeterminate as 'theory'.) Despite these considerations to the contrary, some have thought educational theory to be an array of practical syllogisms.

It is well known that practical reasoning is something that we engage in (or ought to engage in) when trying to answer the question: "What ought we to do?" I quote von Wright for a concise characterization of the form of practical reasoning.

> Aristotle's own treatment of the topic is very unsystematic and his examples are often confusing. One way of reconstructing the main idea here is the following: The starting point or major premise of the syllogism mentions some wanted thing or end of action; the minor premise relates some action to this thing, roughly as a means to the end; the conclusion, finally consists in the use of this means to secure that end. Thus, as in theoretical inference the affirmation of the premises leads of necessity to the affirmation of the conclusion, in a practical inference assent to the premises entails action in accordance with them.[20]

As Petrie has pointed out, the vertical model of the practical syllogism presumes that we can move from the most specific kind of judgment to the most general--the major premiss of the lower-order syllogism appearing as the conclusion of the syllogism immediately above it in a hierarchy of decreasing specificity. An alternative arrangement of practical syllogisms

is horizontal, so to speak, according to which "any given practical syllogism is treated as an explication of a single reason for (or against) a certain value judgment. . . . These are typically reasons for a certain value judgment and reasons against it. There reasons must be weighed together in coming to a conclusion as to what ought to be done, "all things considered."[21]

It is important to realize that at best such systems of syllogisms are useful only in contexts where we have to decide among different courses of action. Specifically, in the context of educational research, the goals of research could be determined in this way. However, the model of the practical syllogism is irrelevant to those methodological decisions that cannot be immediately identified as involving moral choices among alternatives.

The vertical model of practical syllogisms has been specifically criticized as irrelevant to educational decision-making. Scriven notes that

> It is widely supposed that a man's system of values can be thought of as a pyramidical hierarchy, or conversely, as a tree structure. These have, at one end, a large number of specific practical values (liking today's issue of The Times . . .) which are explicable (or justifiable or derivable) from a smaller number of more general values (liking the most compendious paper in the country . . .) which are themselves instances of still fewer and more general values (liking the qualities of being well-informed . . .). Now if this were a realistic account, all one's values would derive from a relatively small number of 'highest' (or 'most basic') values, which by definition are not derived from any other values.. Where do they come from? It seems very plausible--if one is thinking in terms of this model-- to suppose that they must be simply a free choice by the individual. The model cuts them off from any visible means of support, and in doing so it misrepresents the extensive

> interaction between values and experience that actually exists. . . .
>
> Values consist in or arise from needs and wants.[22]

It is not clear that Scriven would apply the same criticism to the horizontal model of practical syllogisms. At least the objection that values are assumed to be cut off from interaction with experience would not readily apply to the horizontal model. But the general objection that we rarely, if ever, proceed to derive decisions for action either in accordance with the vertical or the horizontal model would stand. Either form of the system of practical syllogisms may be adequate for justifying research or actions taken or contemplated, but neither is likely to serve well in generating worthwhile goals for research. We must conclude that those who seek a normative basis for educational research through the model of the practical syllogism may be referring either to the sorts of arrays just described or (since nobody has yet produced a complete system of this sort) they may be referring only to the moral principles which they take to be candidates for membership in some such system of practical syllogisms.

I shall now return to the consideration of the views of those who have in various ways issued the call for the development of a normative basis for educational research. Because the development of an educational theory, either in the formal sense described in (2) or in the informal sense described in (3), although not logically impossible, is a very

long range proposition (and known to be such by those whose views we are considering), we may justifiably conclude that they are not using 'normative basis' to refer to such a theory or to the set of moral principles that might be embedded in such a theory. Instead it is reasonable to interpret their use of 'normative basis' along the lines of (1) on page 15 above. But let us first consider in some detail the background concerns expressed by Gowin and Dunkel in their contributions to the NSSE Yearbook. Two important strands can be discerned easily. One is the dissatisfaction with positivist methodology, particularly with the hypothetico-deductive paradigm and with the requirement that the language of observation remain free of intentional terms. The other strand is, in Gowin's words, the need to make educational research "distinctive", presumably in order to prevent irrelevant research undertakings--to block uncritical acceptance of what frequently turn out to be no more than educational research fads.

It is clear that Gowin, and one may presume Dunkel too, requires that the antiseptic behaviorist observation language be dropped. Thus Gowin, in criticizing the positivist conception of direct observation employed in research on the acquisition of mathematical understanding, says

> While we can say that in a sense the relation between evidence and assertion is made clear (a child understands arithmetic if a number of test items are passed), the advice has the bad consequence of leading us away from, rather than into, the educational phenomena of interest.[23]

I think that it is true that positivists have not provided sufficiently good reasons for avoiding an "enriched" terminology at the level of observation. (This sheds some light on Gowin's complaint, quoted earlier, that "the usual procedure. . . derives grounds for acceptable scientific research from the philosophy of science." Obviously, the kind of philosophy of science that Gowin is referring to is one dominated by positivist methodology, particularly by its dogma about the theoretic neutrality of observation language.) As several philosophers writing on the theory of action and the philosophy of the social sciences have pointed out, it is possible to formulate observation statements of actions. Of course, such statements require the use of intentional language and in every case presuppose certain norms (possibly moral norms which enable us to construe the event as an action. But to agree that the observation language of educational research should be enriched or upgraded to permit descriptions of actions is not at the same time necessarily to agree with Gowin that the behaviorist restriction of such language to references to events inferior to actions "has the bad consequence of leading us away from, rather than into, the educational phenomena of interest." Of course, the expression 'leading us away from' is hopelessly vague. If what is meant by it is that using behaviorist observation language leaves us with what now appears to be an enormously difficult task of inferring from such sparse descriptions something about

cognitive processes of human beings, then Gowin has not claimed anything that most behaviorists would deny. (I should note, however, that radical behaviorists -- Skinnerians--do not aspire to the making of such inferences. They also profess not to be interested in any sort of explanation that is not symmetrical with prediction.) But if Gowin means by 'leading us away from' that the production of scientific knowledge about educational phenomena is impossible unless we not only employ intentional descriptions of actions but also presuppose certain substantive theses about what is educational, then he is unnecessarily importing partisan moral principles into the methodology of educational research. True, the dramatic failure of positivist methodologies to produce systematic knowledge about the higher cognitive processes is sufficient ground for shifting to an intention-enriched observation language but this does not show that behaviorist approaches are conceptually incoherent. One of the main contentions of this paper is that this methodological shift should be restricted to the inclusion of a certain type of observation statement in research--namely, the action description. To be sure, there is no guarantee that all of the normative presuppositions of such a liberalized methodology are non-moral. But there seem to me to be important reasons for recommending that the moral presuppositions of research methodology be kept to a minimum.

The comparison between the position that I favor and that of Gowin and Dunkel can be clarified by contrasting both with the so-called Verstehen doctrine in the philosophy of social science. According to

this doctrine a social scientific explanation must be expressed in terms of the commonsense categories which the actors themselves would employ to describe their own behavior. In its weakest form the doctrine holds that these commonsense categories recognized by the actors must be used to initially mark out or identify the subject matter to be explained. Beyond this point the social scientist is free to erect categories of explanation which may be alien to the conceptual framework of the subjects of the inquiry.[24] This position is clearly expressed in the following passage from Alan Ryan's Philosophy of Social Science:

> . . . the identification of the events to be understood necessarily depends on understanding the rules which make them count as events of whatever kind it may be. Thus when we describe a set of actions as praying, this necessarily is to employ religious criteria, when we describe an act as that of voting this necessarily is to employ political criteria.[25]

I think that Gowin and Dunkel would endorse the addition of the following sentence to the above: "When we describe an act as that of teaching this is necessarily to employ educational criteria." It is true that someone's partisan criteria of what counts as educational are used whenever an act is described as teaching. On this point there is no disagreement between my view and that of Gowin and Dunkel. The point of difference between us is revealed by the question of whether the methodology of educational research or, to use Gowin's expression, the theory of educational research, ought to be made "distinctive" by the

adoption of some partisan concept of education. Dunkel has complained that educational research has been "heeling in the breeze of every whim or rhetorical blast" and therefore needs a normative base. But the most that could justifiably be advanced as a call for a normative base is that researchers ought to be aware of the normative presuppositions of actual research undertakings --a non-controversial recommendation. If more than this is intended then certain unacceptable consequences follow; I shall detail these shortly.

To continue to contrast my view with the Verstehen-type position that I have attributed to Gowin and Dunkel, I should note that mine is weaker than theirs. According to my view, the requirement that intentional descriptions of actions be permitted is sufficient to establish the observational language component of the methodology of educational research. By contrast the Verstehen doctrine holds that an adequate description or explanation must be given only in terms of the commonsense categories of the agents whose actions are being described or explained. In our culture this would include admitting terms referring to beliefs, feelings and the like--all intentional terms. I characterized the Gowin-Dunkel position as being of the Verstehen type because of its logical property of excluding descriptions of events inferior to actions as educationally irrelevant. (On my view such descriptions are not excluded despite the fact that they may occur rarely in ordinary talk about educational phenomena. But if we go beyond this similarity, some unclarity appears about whether the Gowin-Dunkel thesis requires the isolation (after suitable rational deliberation, we may presume) of a particular set of descriptions for

picking out educational phenomena (which would not be consistent with the Verstehen view), or whether it requires simply that whatever substantive concept of education is held by persons whose behavior is being investigated be reflected in the descriptive and explanatory statements of the researcher (this is consistent with the Verstehen view).

My position should also be contrasted with that of Popp which was discussed earlier. His thesis is stronger than mine with respect to the claim about the ethical or moral neutrality of educational research. Popp holds that (1) the methodology of research is morally neutral and that (2) actual research designed solely in the light of epistemic utilities is morally neutral. On my view it is (a) possible and likely, but not certain, that the methodology of research remain morally neutral and (b) not possible in actual research to avoid moral commitment because such commitment resides in the very choice of the set of observational categories employed.

Now on first look it may appear arbitrary to draw the line between research methodology and functions extrinsic to it in the way that I have done. One might object that since the specification of every research design that contains action descriptions unavoidably favors one set of substantive moral values over alternative sets, then such inescapable decisions may as well be construed as part of the methodology of research. But to adopt such a recommendation would be to introduce an indefinitely variable element into the methodology of research because the individuation and description of events as actions is itself indefinitely variable. Such

a result would be inconsistent with the very concept of a methodology as a set of rules. It is not clear whether adopting the Gowin-Dunkel thesis commits one to this inconsistency or whether their thesis requires us to decide substantive moral issues at the level of the concept of education and then to incorporate that into the methodology of research. To act on the second interpretation would be to set up barriers to the debate of such moral issues whenever a particular research design is proposed. What is worse, it would curtail the unfettered expression of the partisan moral preferences of the various groups in the society, each of whom has a right to influence educational research. The unfortunate fact that certain interests of minorities are frequently not considered because of the successful opposition of other groups who advance their educationally relevant values is a good reason for keeping the methodology of educational research as free of moral value commitments as possible. To do otherwise is at least to increase the likelihood that the values or ideals of some group in the society may enjoy a competitive advantage over alternative and conflicting ideals under the protective label "methodology of educational research". The kind of "distinctiveness" that Gowin would like to see educational research acquire and the sort of stability and direction that Dunkel seeks for it would have been purchased at a considerable price. Furthermore, a good reason for reducing the moral commitments of research methodology is the need to provide on-the-job training in moral deliberation for the large number of educational researchers involved in local projects.

To make possible and to encourage such deliberation might remove some of the danger from incompetent handling of moral issues that was made possible, if not encouraged, by widespread adherence to the myth of a value-free social science.

II

1. I shall continue to take it for granted that curriculum research is included in educational research and that therefore what I have said regarding the normative basis for educational research applies to research in curriculum. Thus if the methodology of curriculum research is to avoid foreclosure of certain normative issues that must be resolved in the course of a particular curriculum research effort, then we should expect that the concept of curriculum development which such a methodology employs has to be sufficiently indeterminate to accommodate partisan and competing explanatory schemes. If this guideline is not observed we run the risk of establishing a new methodological orthodoxy. It is with this general point in mind that I shall examine a recent approach in curriculum research that seems to embody the two methodological characteristics that Gowin and Dunkel require for educational research--the upgrading of descriptive terminology to include the level of action and "distinctiveness."

The approach to which I refer has been called by its proponents "empirical" or "naturalistic." I believe that these labels have their origin at least as far back as Schwab's The Practical where there is

a strong plea to researchers to go "back to the phenomena." For instance:

> What is wanted is a totally new and extensive pattern of empirical study of classroom action and reaction; a study, not as a basis for theoretical concerns about the nature of the teaching or learning process, but as a basis for beginning to know what we are doing, and to what effect; what changes are needed, which needed changes can be instituted with what costs or economies, and how they can be effected with minimum tearing of the remaining fabric of educational effort (emphasis in original). [26]

Elliot Eisner has also taken this stance, but with particular reference to curriculum research.

> It has long been apparent that while the field of curriculum has many educational scholars who have contributed mightily to the conceptual aspects of the field . . . its empirical aspect, that is, the study of processes central to curriculum as a field of study, has been neglected. When one stops to realize that the recommended procedures for building educational programs have been based upon little or no actual study of the process of curriculum development, one cannot help but wonder about the validity of such recommendations. Thus, it is apparent that while the conceptual systems that have been advanced so admirably by Tyler, Goodlad, and others are necessary for conducting inquiry into curriculum problems, without empirical test they lack the grounding necessary for building the field as an area of scholarly study and artful practice. [27]

As a final example of this methodological orientation let me quote Richard Hawthorne and Decker Walker. Hawthorne says

> It must be made clear that prescriptive theories of curriculum activity are of obvious necessity if one accepts that the central questions to be addressed in the design, development and evaluation of curricula are of a moral and political rather than empirical order (my emphasis) p. 2. [28]

Walker, in discussing the shortcomings of the Tyler rationale, remarks that

> The classical model is, of course, intended to be prescriptive rather than descriptive, but those who recommend it as a norm imply thereby that practice guided by the model does what ordinary practice does, only better.[29]

I think that it is a fair assessment of the views represented in the preceding quotations to say that the authors endorse a separation between the conceptual, the normative, the prescriptive, and the moral, on the one hand, and the empirical, the factual and the descriptive on the other. I shall contend that maintaining this alleged separation between fact and value has led them to advocate what appears to them to be a value-free (i.e., merely descriptive or "naturalistic") curriculum research program. But I shall argue that it carries normative presuppositions just as the Tyler rationale does.

I shall try to ferret out the normative presuppositions of this so-called empirical or naturalistic approach to curriculum research by considering the work of Decker Walker who has developed in some detail the recommendations made by Eisner. I shall follow my analysis with some conclusions about the nature of the normative basis for curriculum research. This in turn will make it possible for me to show exactly in what respect the Eisner-Walker objection to the Tyler rationale is misleading.

Eisner's proposal was to use the processes of curriculum development, implementation and evaluation as the empirical base for curriculum research. Walker's several published papers have elaborated on this with respect to the first of the three processes. His research meets the demand for distinctiveness in that observations are made only of the deliberations of curriculum development projects; it meets the demand for a liberalized observation language by employing categories that can accommodate the construal of sets of events as human actions. But if we recall the arguments of Part I of this paper, it is apparent that the choice of any particular set of intentional observation categories, however defensible, brings along with it unavoidable normative commitments of one sort or another. Hence we should be suspicious of the labels "naturalistic" or "empirical" if these terms are used so as to contrast with the prescriptive and the value-laden. What then are Walker's descriptive or observational categories?

Walker employs a content analysis system and applies it to transcriptions of deliberative discourse recorded in actual curriculum development project meetings. There are several levels of analysis. At the so-called macroscopic level of analysis, four types of units called episodes are distinguished: issues, reports, brainstorms, and explications. The next level of analysis, the microscopic, sorts the discourse of the curriculum developers into various types of deliberative moves: problems, proposals, arguments, instances. Acceptable inter-judge agreement was

obtained in the application of the categories of both the macroscopic and the microscopic analyses to actual discourse. Statistical techniques were employed to draw a variety of conclusions about curriculum development discourse. For instance, issues turned out to be by far the most frequently occurring episodes, the various deliberative moves occurred in the same proportion in issues as they did in the entire transcript, and what Walker calls the "basic deliberative pattern"--a problem followed by several proposals, each receiving several arguments--could be discerned throughout the transcript. Some of the results seem less than interesting because it is doubtful that they represent empirical discoveries. Here are some examples:

> The four types of episodes displayed the expected differences in rates of interaction, with issues and brainstorms showing a high rate of interaction among planners, and explications and reports a low rate of interaction.
> Those portions of transcript that did not fall under any episode seemed to contribute little to the curriculum making task, consisting mostly of discussions too brief to qualify as episodes.[30]

> . . . brainstorms showed more proposals and fewer arguments, and explications showed markedly fewer arguments (emphases in the original throughout).[31]

These conclusions seem to follow from the very meanings of the terms that are used to express them. That is, part of what we mean by describing something as an "issue" or a "brainstorm" is that, in suitable contexts, it produces a high rate of personal interaction. Again, if brainstorms were indeed more like arguments and less like proposals, then the word

'brainstorm' would not have the ordinary meaning that it does now. Empirical research appears unnecessary either for warranting or for producing such truths.

The third stage of Walker's system is a fine-grained analysis of one of the products of the second stage--the argument. In this stage a classification scheme is used for sorting the sources and the content of the data on which the argument depends.

We can infer from what Walker says that the breaking up of the stream of deliberative discourse (by sorting it into intentional categories of varying degrees of specificity) has both a proximate and a long-range purpose. The sorted data generated by a particular project may be of immediate benefit to the project evaluator who is attempting to pinpoint the causes of deficiencies in the product or in the process of development. (Of course, being able to use the data for this purpose presupposes that the evaluator has independent and clear criteria for what counts as a good product and as a good development process.) Walker's assessment of the long-range benefits is contained in the concluding remarks of his study:

> . . . there is another very different and, to my mind, quite promising sort of inquiry one can conduct with the aid of a system like the one used in this study. It is a detailed critical study of particular portions of actual deliberation, in all their peculiarity and uniqueness. I believe that both of these approaches to the study of the process of curriculum deliberation can contribute significantly to practice, theory and research in the field of curriculum.[32]

In other words, according to Walker, we ought not only to undertake a very fine-grained classification of deliberative discourse but also to utilize the categorized data as the basis for further research which, if successful, would lead to a theory of curriculum development. I think that it may be instructive to speculate about Walker's use of the phrase 'in all their peculiarity and uniqueness.' That is, why is he recommending that we press toward an increasingly fine net of classifications of deliberative discourse? One argument against accepting this recommendation is that the more fine-grained the sorting of curriculum discourse becomes, the more closely it approximates an ordinary narrative account of the deliberations--an account which could be produced by an experienced and sensitive observer using the categories merely as a checklist. Such a classification scheme would take on the unwieldiness of informal analysis--the recommended procedure would leave us at the level of common sense and provide no boost to our theoretical or research efforts. Of course, a reason for undertaking the fine-grained analysis of Walker's Phase III would be the failure to find significant relationships among the data sorted according to the categories of Phases I and II. To my knowledge, such relationships have not yet been systematically sought; hence a failure to find them could not be the motive for recommending a Phase III type of analysis. I suspect that the motive for the recommendation is an implicit adherence to the empiricist methodological dogma that there are curriculum phenomena

"out there" to be discovered if only we are resourceful enough to construct the right sort of grid to capture them. If I am correct, this is precisely the belief that accounts for Walker's characterization of his position as "naturalistic" and for the recommendation of Hawthorne, who cites Walker's work with approval, that "we give a higher priority to the empirical study of curriculum phenomena for the purpose of comprehending their essence (my emphasis)."[33] But as we have seen in Part I, any set of classificational schemata necessarily leaves out some things as irrelevant or unimportant while fixing on certain others. That is, our normative presuppositions determine what our categories can pick out or what they must leave out. This result takes on particular significance when many of the events to be categorized in the scheme are construed as actions: descriptions of events as actions are indefinitely variable and norms used to specify a particular construal are often moral norms. (For instance, the same event may be construed as either an act of cooperation or of conformity. In the context of schooling the norms determining which construal is to be adopted are moral ones.) There are then no curriculum "essences", only competing sets of category schemata.

2. The discussion up to this point has not touched on the issue of disjointed incrementalism, a strategy of decision making which Walker and others believe may explain the processes of curriculum development. My concern here will not be to test the explanatory adequacy of this partial

model, for that would require, among other things, an empirical investigation which I am not in a position to undertake. (The model is partial since it cannot be used to predict which events will take place during curriculum development; it merely serves to exclude certain kinds of models as candidates for being full explanations.) Instead I shall later provide grounds for concluding that Walker's adherence to the belief that disjointed incrementalism (hereafter 'DI') is an adequate overall model for curriculum development has influenced his choice of the network of descriptive categories. Whereas I have just argued that these categories cannot be merely descriptive if that connotes normative or moral neutrality, I shall be concerned in this section of the paper with attempting to exhibit the respects in which DI and the associated system of observational categories are normative and moral.

'Disjointed incrementalism' is a phrase originated by Lindblom and Braybrooke and is used by them to refer to a particular strategy of decision making in social-political contexts. It is best described by contrasting it with an approach to decision making which Lindblom and Braybrooke vehemently reject, the so-called synoptic ideal--the systematic pursuit of which requires an agreed-upon set of values and a comprehensive overview of policy problems and alternative policies. Despite the allegiance paid to it, the synoptic ideal turns out to be incapable ever of being carried out in practice because it requires an impossible degree of calculation and projection of consequences. There are other difficulties besides this

calculation--in order to carry the synoptic ideal through, one has first to measure the uncertainty that attaches to the prediction of consequences of policy choices and second to come up with a method for interpersonal comparison of preference intensities, both unsolved if not unsolvable problems.

DI is advanced by Lindblom and Braybrooke as the correct alternative to the rationalist synoptic ideal. Here is one critic's brief but comprehensive characterization of DI:

> Major decisions of any sort are to be avoided, and problems are to be dealt with by small steps rather than grand designs. Further, these small steps are not to be the result of efforts to reach major objectives; in fact Lindblom recommends the adjustment of objectives to the policies in some cases (p. 93). The amount of consideration given to each small step is small, with only a few alternatives considered (p. 88) and only a few consequences of the action taken into account (p. 90). The general picture is one of a long series of small steps, each step being taken not as part of a plan to reach some ultimate destination but simply because that particular step seems sensible regardless of the long-range outcome.[34]

Lindblom and Braybrooke argue, in effect, that since DI properly describes the way in which social policy decision-making proceeds in fact, and since what they take to be the only other alternative--the synoptic ideal--cannot be realized, then DI ought to be the strategy consciously followed. But certain normative ethical considerations force them to restrict this recommendation to the satisfaction of meliorative claims as distinguished from peremptory ones.[35] ("On a meliorative approach, judgments about accepting or rejecting any policy must wait upon a comparison of that

policy with alternatives to it. On a peremptory approach, certain characteristics are looked for on the basis of which a policy would be approved or disapproved taken by itself, without any attention necessarily being given to alternatives.")[36]

But the restriction to meliorative values represents a significant limitation on the relevance of DI to the study of the process of curriculum development. To the extent that such deliberations in actual cases are aimed at the satisfaction of peremptory claims, using descriptive categories particularly appropriate to the DI model will fail to produce data which accurately represent what has taken place. The Walker categories, in particular, at least through the first and second phases of analysis, seem inadequate for revealing that peremptory constraints or conditions apply to proposals, especially if these are never referred to explicitely in the course of the deliberations. Instead, it is likely that an analysis in Walker's terms would make <u>every</u> discourse seem meliorative--hence ripe for the application of categories chosen in conformity with the DI model. This is so because the Walker categories obscure the presence of content and attitude patterns that cut across the so-called episodes.

Even were we to concede that curricula are rarely developed in order to meet only peremptory claims, the recommendation to proceed with the DI strategy would be open to a serious objection. Lindblom and Braybrooke tacitly assume that the demonstration that the synoptic

ideal cannot be carried out is sufficient to warrant abandoning the sort of decision procedures which purport to achieve that ideal. Kenneth Arrow has argued against this assumption.

> A synoptic ideal would at least force the attention of decision-makers on as many relevant factors as social scientists, even with our present highly imperfect knowledge, can see as significantly relevant. A "strategic" approach [one which follows DI], relying heavily on self-interest for the production of arguments, can all too easily suppress the claims of the inarticulate and the ignorant. It is precisely the function of the social theorist to point out the unsuspected connection, the interests and implications not apparent on the surface. The synoptic demand for as careful a listing of alternatives and consequences as is possible imposes a criterion of objectivity on social discussion which a wholehearted acceptance of the strategy of disjointed incrementalism would deny.[37]

At least this much is clear from what Arrow says: the methodological decision to adopt the DI strategy carries with it a commitment to a moral norm -- roughly that certain sorts of claims, factors or alternatives must be construed as more important or relevant than others. No doubt this methodological stance is consistent with, and may even be required by, certain overriding moral ideals that many social decision-makers hold. But nevertheless it is also indisputable that with respect to other competing ideals the methodological stance of DI is proscribed.

It is interesting to note that the same type of complaint that we found Kenneth Arrow making about the DI model--that using it to describe

or give an account of decision making distorts or leaves out important things--can also be raised by those who favor DI, except that in their case it is adherence to the synoptic ideal which they claim results in distortion. Thus in criticizing Taba's scheme, one which clearly follows the synoptic ideal, Kirst and Walker say

> Once such an ideal has been adopted, it is difficult to avoid disapproval of political resolutions of curriculum questions. And, once political solutions to curriculum questions are seen as deficient or inferior, the tendency is to lump all the complex and varied means by which personal and group interests are defended and advanced in curriculum issues under the vague and sometimes sinister term "influences" and to treat them as aberrations rather than as normal and necessary, if not altogether desirable, aspects of public policy-making.[38]

It is not clear what they mean by 'normal and necessary, if not altogether desirable.' Does it mean that political power is wielded in certain inescapable ways ("necessary" in the sense that alternative ways invariably fail) even though we may not approve of this from a moral point of view? If so, then the possibility of raising moral objections to the endorsement of DI has not been blocked--in fact, it has at least prima facie validity because to adhere to DI would seem to be to pursue a conservative policy of social change in most instances.

Walker and Kirst commit themselves to the claim that the best way to describe the way curricula actually get made is to say that curriculum developers "adopt what Lindblom and Braybrooke (1963) called a strategy of disjointed incrementalism;" that "curriculum decision-makers use informal methods of decision-making."[39] I suspect that

Walker, for one, would want to hold that his commitment is limited to the use of DI as a descriptive model of the process of curriculum development. That is, he might want to maintain that once we have adequately described what happens--something which, according to Walker and Eisner, the followers of Tyler and Taba have not done--then we will have the empirical ground for prescribing how we ought to go about developing curricula. I shall consider these two points then: (1) the objection to the Tyler rationale and (2) the respect in which DI and Walker's system of observational categories are normative and moral rather than being merely descriptive.

(1) It is misleading to object, as Walker and Eisner do, that as a prescription for curriculum development, the Tyler rationale was and continues to be advanced without the benefit of knowledge of how actual curricula get built. Taken literally, this objection is clearly too strong if not false, since it is reasonable to suppose that Tyler and some of the others who endorse the rationale possess a great deal of experience in the development of curricula. What is true, of course, is that they have not undertaken investigations of the sort that Walker hopes will produce knowledge which is more comprehensive than the informal knowledge about curriculum development upon which the Tyler rationale was based. But to claim in advance that a particular system of classifying curriculum deliberations will get us beyond the informal knowledge we already possess is unwarranted. The "empirical" objection to the Tyler rationale appears stronger than it is only because it tacitly assumes that the use of "naturalistic" observation

procedures will eventually disclose the curriculum development phenomena which are "there to be discovered." As I have argued earlier, this claim cannot be sustained.

(2) An examination of Walker's first two phases of analysis reveals that in addition to the shortcomings already described, they are inappropriate for use in discerning certain kinds of phenomena which may be important for the advance of curriculum development research. For reasons already mentioned, such factors as alliances among participants, the operation of subconscious psychological forces and the dominance in the development group of an ideology (e.g. Marxism) would be missed by the application of Phase I and II analyses. By contrast, these analyses appear particularly well suited for discourse that is characterized by discrete and relevant reactions of autonomous participants to equally discrete, relevant and serious remarks made by issue-oriented speakers who are themselves autonomous. Indeed it is difficult to avoid the conclusion that the points just made are true also of the DI strategy. It would seem that at least the Phase I and II observational categories may have been chosen by Walker so as to facilitate description according to the DI model. If this is correct, then to list (as I have done) some types of phenomena which can, and some which cannot, be coherently described within Walker's system is to illustrate the normative presuppositions of both that system and the DI strategy to which it conforms. In order to prevent misunderstanding I should emphasize that I am not

endorsing some set of categories as an alternative to Walker's, nor am I implying that his choice of Phase I and II categories is indefensible. Instead I claim merely that Walker's scheme cannot be normatively neutral, and ask the reader to consider, for instance, how different from those of Walker's system would be the normative presuppositions of the observational categories likely to be employed by, say, a neo-Freudian psychologist who tried to establish causal explanations within curriculum development processes. It is only because of the social scientist's metatheoretical bias toward something like DI that he would perceive the Freudian researcher's position as non-neutral while the neutrality claims that might be made for categories such as Walker's would go unchallenged. The arguments of this paper have sought to establish that because every classification scheme unavoidably excludes normatively significant alternative formulations, all neutrality claims for such systems are untenable.

It remains to be asked whether Walker's fine-grained Phase III analysis provides for the formulation of descriptions of the sort of phenomena that I have claimed cannot be described by the categories of Phase I and II. Possibly. Let us assume that it does. But then this is the result that we should expect because, as I have argued earlier, Phase III comes perilously close to being functionally equivalent to a comprehensive but informal ordinary-language analysis. It is complex and hence too unwieldy to be a fruitful source of data for theory and research (although it may well be useful for evaluating a particular

curriculum project). Because so much can be expressed by the categories of Phase III, it has been rendered theoretically uninteresting. There is no suppression of detail to reveal pattern, the hallmark of any theoretically fruitful description. To establish its normative presuppositions becomes equally uninteresting and pointless.

A final note: I have claimed at several points in this paper that in social scientific methodologies the normative presuppositions are likely to be moral as well. I want to substantiate this claim by means of an example which further builds upon the contrast between the set of observational categories that Walker employs and the very different set likely to be used by, say, a neo-Freudian psychologist doing research in order to improve curriculum development. For purposes of the example let us stipulate that both researchers judge a curriculum development project successful if it comes up with a product which the members of the development group will agree on, support and promote. The prescriptions for improvement generated by both researchers are designed to expedite the process of reaching agreement. Is it reasonable to expect morally significant practical differences between the curricula developed according to these prescriptions? Of course such differences may not appear, for very dissimilar causal chains may result in identical events. But it seems more likely that the products would be markedly different. One might conjecture that successful prescriptions formulated in terms of Walker's categories (e.g., to deliberately increase the frequency of

brainstorms vis a vis issues) would contribute to the desired agreement by expediting bargaining and compromise during deliberation. By contrast, the prescriptions of the psychoanalytically-oriented researcher would put a premium on the emergence, say, of empathic relationships that would facilitate persuasion--i.e., the alteration of belief-structures among group members. Curriculum products developed in these two cases are also likely to reflect the differences in the prescriptions for changes in the development processes: In the former case the result might be a rather eclectic package reflecting the demands of "interest groups", while in the latter it is likely to be more self-consistent, perhaps representing the interests of the most persuasive/rational/calculating members of the development team. In other words, the use of prescriptions couched in the different terminologies of the two contrasting sets of observational categories marks out different sets of available options for using power within a curriculum development project. This is only one example of the way in which the choice of descriptive categories might have moral consequences. If a curriculum theorist faced with these methodological choices either knows, or is in a position to come to know, what the differences between the moral consequences of alternative methodologies are, then his choices are inescapably moral ones.

NOTES

[1] D. Bob Gowin and Jason Millman, "Research Methodology--A Point of View," RER, Vol. 39, No. 5, p. 553.

[2] See for instance Elizabeth S. Maccia and George S. Maccia, Development of Educational Theory Der ed from Three Educational Theory Models, The Ohio State University Research Foundation, Columbus, December 1966.

[3] Joseph Margolis, Values and Conduct (Oxford: The University Press, 1971) p. 8.

[4] According to Margolis appreciative judgments are taste-bound, that is, the predicates that are employed in such judgments "are applied to things only on the condition of the judge's having a certain commensurate taste, personal conviction or preference." Findings, however, "do not presuppose appropriate taste on the part of the judge." Instead they "behave in precisely the same way as factual judgments, that is, there are recognizably public grounds on which to test the truth of findings." The distinction between findings and appreciative judgments does not correspond to the traditional distinction between moral and aesthetic judgments. Instead, it cuts across these categories--hence its usefulness in discussing normative issues in education.

[5] Margolis, op. cit., Chapters 3 and 4.

[6] Michael Scriven, Value Claims in the Social Sciences, Publication #123 of the Social Science Education Consortium, Purdue University, 1966, p. 32.

[7] Jerome A. Popp, "Significance and Utility: A Study in the Inductive Problems of Educational Research," Proceedings of the Twenty-Seventh Annual Meeting of the Philosophy of Education Society, 1971, pp. 176-177.

[8] Ibid., p. 179.

[9] Scriven, op. cit., p. 34.

[10] Richard S. Peters, "Education and the Educated Man," Proceedings of the Annual Conference, Philosophy of Education Society of Great Britain, Vol. 4, January, 1970.

[11] G. Reddiford, "Conceptual Analysis and Education," Proceedings of the Philosophy of Education Society of Great Britain, Supplementary Issue, Vol. VI, No. 2, July 1972, p. 195.

[12] Lawrence G. Thomas (ed.) Philosophical Redirection of Educational Research, The Seventy-first Yearbook of the NSSE, Part I (Chicago: The University of Chicago Press, 1972) p. 3.

[13] D. Bob Gowin, "Is Educational Research Distinctive?" in Thomas (ed.) op. cit., p. 9.

[14] Ibid.

[15] Ibid., p. 16.

[16] Ibid., p. 23.

[17] Harold B. Dunkel, "Wanted: New Paradigms and a Normative Base for Research," in Lawrence G. Thomas (ed.) op. cit., p. 93.

[18] Brian Holmes, "Conceptions of Culture and Society in Educational Research on Individuals," in Lawrence G. Thomas (ed.) op. cit., p. 216.

[19] For a lucid account of this type of explanation see Carl G. Hempel, Philosophy of Natural Science, (Englewood Cliffs: Prentice-Hall, 1966), pp. 47-54.

[20] Georg Henrik von Wright, Explanation and Understanding, (Ithaca, New York: Cornell University Press, 1971) p. 27.

[21] Hugh G. Petrie, "Practical Reasoning: Some Examples," Philosophy & Rhetoric, Vol. 4, No. 1, Winter 1971, pp. 33-34.

[22] Scriven, op. cit., pp. 20-21.

[23] Gowin, op. cit., p. 21.

[24] Michael Martin,"Explanation in Social Science: Some Recent Work," Philosophy of the Social Sciences, Vol. 2, No. 1, March 1972, p. 77.

[25] Alan Ryan, The Philosophy of the Social Sciences (New York: Random House, 1970) p. 143.

[26] Joseph J. Schwab, The Practical: A Language for Curriculum (Washington, D. C., NEA, 1970) p. 31.

[27] Elliot W. Eisner, "Curriculum Development: Sources for a Foundation for the Field of Curriculum," Curriculum Theory Network: 5, Spring 1970, p. 7.

[28] Richard D. Hawthorne, "An Empirical Approach to Curriculum Theory Building," paper presented at the AERA National Convention, January 1971, New York City.

[29] Decker F. Walker, "A Naturalistic Model for Curriculum Development," School Review , Vol. 80, No. 1, November 1971, p. 64, fn. 2.

[30] Decker F. Walker, "A Study of Deliberation in Three Curriculum Projects," Curriculum Theory Network : 7, 1971, p. 122.

[31] Ibid., p. 127.

[32] Ibid., p. 133.

[33] Hawthorne, op. cit., p. 3.

[34] Gordon Tullick, Review of A Strategy of Decision: Policy Evaluation as a Social Process, by David Braybrooke and Charles E. Lindblom, Ethics, Vol. LXXV, No. 1, p. 68.

[35] David Braybrooke and Charles E. Lindblom, A Strategy of Decision (The Free Press of Glencoe, 1963) p. 218.

[36] Ibid., p. 150.

[37] Kenneth J. Arrow, Review of A Strategy of Decision, Political Science Quarterly, Vol. LXXXIX, No. 4, December 1964, p. 587.

[38] Michael W. Kirst and Decker F. Walker, "An Analysis of Curriculum Policy-Making," RER, Vol. 41, No. 5, December 1971, p. 482.

[39] Ibid., p. 485.